

children than some that are indulged in. These I believe to be the views of nearly all teachers as well as those of myself, who am but a

SCHOOL TEACHER

December 8

The Tokio Earthquake of October 15, 1884

AT 4h. 21m. 54s. a.m. the inhabitants of Tokio were awakened by a sudden and violent earthquake. In Yokohama, which lies about sixteen miles south-west by south from Tokio, the disturbance was noted at 4h. 21m. 38s., that is to say, sixteen seconds before it was felt in Tokio. The chief source of error in these time-records—if error exists—will probably be due to observers at different stations having noted time at different portions of the disturbance, the length of which, as determined by the sensations of those who made the records, was about one minute, but, as recorded by a seismograph, between five and six minutes. At the commencement of the disturbance four complete waves were described in three seconds, but at the end of the disturbance the motion became so slow that each wave occupied from two to three seconds. From a record taken by Mr. K. Sekiya, a gentleman whose especial duty it is to attend to the earthquake phenomena of this country, it would appear that the maximum range of motion may have reached 42 mm. The maximum acceleration per second per second was about 500 mm., that is to say, the intensity of the earthquake or its destructive power was similar to that which would be experienced by a building standing on a carriage which was suddenly caused to move with a velocity of about one foot and a half per second, or if, such a carriage having gradually acquired such a velocity, it had been suddenly arrested. The result of the earthquake was to overturn a few chimneys in Yokohama and to crack one or two in Tokio.

Our last severe earthquake was on February 22, 1880. On that occasion in Yokohama very many buildings lost their chimneys and were unroofed, whilst in Tokio the damage was chiefly confined to loosening tiles and shaking down plaster. Had our buildings in Japan been constructed like those in England, it is probable that this last shake would have caused about the same amount of damage as that which was so recently caused by the late disturbance in Essex. From the observations on direction, coupled with what has been said about time, it seems that the earthquake had its origin in Yedo Bay, at or about the same point as that which was determined for its severe predecessor.

It may here be remarked that nearly all the heavier earthquakes which are felt in Tokio and Yokohama practically have had a common centrum. They are not large earthquakes as measured by the area shaken, but they are severe because we are near to their origin.

The earthquake of 1880, according to a record furnished by one of Palmieri's instruments, had an intensity of 78°, whilst the recent earthquake, the actual intensity of which, as deduced by its destructive effect, was much less, is given as 95°.

These intensities measured in degrees really indicate the height to which a certain quantity of mercury in a bent tube was caused to wash—the height of the “wash” being measured by the turning of a pulley connected by a string to a small weight floating on the surface of the mercury. It would seem evident that the magnitude of the records obtained in this manner must among other things depend upon the duration of the earthquake, the period of its waves, and the depth of the mercury contained in the tube. For reasons such as these, records like those just given cannot be regarded as anything more than roughly approximative.

In connection with the remarks made on the amplitude it may be stated that the seismograph by which the record was taken was situated on soft soil in the flat portion of Tokio. This amplitude, had it been recorded on the hard ground of a hill, probably would not have exceeded 25 mm.

One of the most remarkable points connected with this disturbance were the changes in level as observed by the displacement of specially arranged pendulums, which took place before the shock, and again about six hours afterwards.

J. MILNE

Large Meteor

ONE of the largest meteors that I have seen for some years appeared at 7h. 15m. 15s. this evening. It began as a speck, north of Vega, at about 4° greater altitude than that star. The course was perpendicularly down, only disappearing by passing

below the horizon. It was 2° east of Vega on descending to the altitude of that star, and by that time had increased to fully a quarter the apparent size of the moon, and this size it maintained whilst above the horizon. The colour was an intense blue, and there was left a streak of orange-red elongated separate stars in its track, and this streak was about 1° in length, although the separate stars of which it consisted disappeared almost as rapidly as they were formed. The stars, like the meteor, increased in size and brilliancy from a mere point, and instantly vanished on attaining their maximum brightness. Each moved perpendicularly down for the length of about half a degree, and left a continuous momentary streak. None of these stars were seen within half a degree of the meteor, and their ignition was confined to the centre of the meteor's path. Their size was tolerably equal, being about that of a second-magnitude star. The speed of the meteor was unusually slow, it being visible for nearly six seconds. The shape was circular in front and cuneate behind (bluntly conical). Its brilliancy was great, considering the presence of a nearly full moon.

Shirenewton Hall, near Chepstow,

E. J. LOWE

December 4

The Cost of Anthropometric Measurements

ALLOW me to correct an absurd typographical blunder in the account of my anthropometric laboratory at the Healtheries, which appears in Mr. Ernest Hart's lecture at the Society of Arts. It originally occurred in the *Journal* of the Society of Arts, whence it was copied into your columns (p. 142) last week. The effect of the error to which I refer is to make the statement that the cost of measuring each person at the laboratory in seventeen different ways was 3*l.*, whereas it should have been 3*d.* The subsequent argument, based on the extreme cheapness of the process, becomes in consequence unintelligible. I write myself to make the correction, because the part of Mr. Ernest Hart's address which refers to the anthropometric laboratory was written for him, at his request, by myself. I regret I had not an opportunity of revising it in proof.

FRANCIS GALTON

The Northernmost Extremity of Europe

As “a Norwegian” now fully admits that the pretended discovery of Capt. Størensen is no discovery at all, but an elementary fact well known and long known to Norwegian geographers, I need not discuss that question any further, but I must protest against his reference to Sönsberg's “Norge,” which is the joint production of some of the most eminent men in Norway. Sönsberg is the *editor* and *publisher*.

Amongst the writers who have co-operated to produce the national “Handbook” are the following:—Lieut.-Col. Broch, Chief of the Geographical Survey of Norway (he is the largest contributor, and the writer of the words I quoted), Prof. H. Mohn, Prof. T. Kjerulf, Prof. Rasch, Prof. L. K. Daa, Sørenskriver, H. Thoresen, J. B. Halvorsen (the well-known writer), Beaureauechef Kjer, and Secretary Mohn, Th. Bock (Royal plenipotentiary), J. N. Prahm, Capt. Scharffenberg, E. Mohn, Lieut. Flood, Capt. Overgaard (the Inspector of Forests), Hörbye, Lieut. Langeberg, and Mr. Langeberg, K. Lassen, Dr. Kahrs, Lieut. Solem, O. T. Olsen, Capt. Bang, Capt. Haffner, and Sørenskriver Nannestad.

All these names are given in the preface, and the contributors of each carefully specified. This was known to “a Norwegian” when he wrote his last letter, for he refers to that same preface, and yet asserts that Sönsberg “never claimed the least *geographical* authority for a faulty and crude guide to tourists” (his own italics). That preface is written for the express purpose of claiming such authority and thanking the authors. It makes special claim in a special paragraph of the *geographical* authority of the “head of the Geographical Survey,” Lieut.-Col. Broch, whose name, Sönsberg says, “offers a sufficient guarantee of correctness.”

The anonymous “Norwegian,” in further disparagement of the book, states that in this preface “the author himself says that for reasons explained it has many faults.” I will quote this very damaging confession. It is as follows:—“A few errors and misprints will be found here and there.” A list of them is given. After this the flippant misrepresentation of my pretensions in the last paragraph of the letter is not surprising, and demands no further notice.

I make this protest, knowing that NATURE is largely read by

well-educated Norwegians (who all read English as a matter of course). They cannot fail to be indignant if such unjust treatment of a national work, which genuine Norwegians understand and appreciate, is allowed to pass unrefuted. Beside which, Englishmen in search of available and reliable information concerning Norway might be grossly misled.

W. MATTIEU WILLIAMS

APOSPORY IN FERNS

A PARAGRAPH in the report in NATURE (p. 119) of the meeting of the Linnean Society for November 20 last contained what is, to the best of my belief, the first publication of one of the most interesting botanical observations which has been made for some time. As it is quite possible that this brief record may escape the notice of a good many botanists, I venture to give the matter a little more prominence.

At the meeting referred to, Mr. E. T. Druery made a second communication (the first did not, I think, receive any record) upon a singular mode of reproduction in *Athyrium Filix-femina*, var. *clarissima*. In this fern the sporangia do not follow their ordinary course of development, but, assuming a more vegetative character, develop more or less well-defined prothallia, which, according to Mr. Druery's observations, ultimately bear archegonia and antheridia. From these adventitious prothallia the production of seedling ferns of a new generation has been observed to take place in a perfectly normal way.

Mr. Druery very kindly offered at the meeting to supply me with some of his material. This reached me on November 29, and I immediately placed it in the hands of my friend Mr. F. O. Bower, who was engaged in other research connected with the vascular cryptogams in the Jodrell Laboratory of the Royal Gardens. Although in the material sent me the abnormal development of the sporangia had not proceeded very far, Mr. Bower obtained evidence which, as far as it went, was entirely confirmatory of the correctness of Mr. Druery's observations. With appropriate cultural treatment prothalliform bodies have already made their appearance, but have not yet reached the stage at which archegonia and antheridia are developed. They are, however, furnished with root-hairs.

This is, however, not all. Mr. Bower placed himself in communication with Mr. Druery, and paid a visit to his collection of ferns. By the kindness of this gentleman he was allowed to bring away specimens of another fern (*Polystichum angulare*, var. *pulcherrima*) which altogether eclipses the *Athyrium*, remarkable as that is. In the *Polystichum* the apex of the pinnules grows out into an irregular prothallium, upon which Mr. Bower with little difficulty was able to demonstrate at Kew the existence of characteristic archegonia and antheridia. In this case the production of the prothallium is not even associated locally with the sporangia, but it appears as a direct vegetative outgrowth of the normal spore-bearing plant. The oophore is a mere vegetative process of the sporophore, a suppression of the alternation of the two generations which exceeds even that which obtains in the flowering plant.

Mr. Druery's discovery, for which I have borrowed Mr. Bower's convenient term Apospory, is the direct converse of the Apogamy in the fern, discovered by Prof. Farlow. In this the sporophore is a vegetative outgrowth from the oophore. The parallel phenomena in the life-history of the moss have been known for some time. But this point and all detailed observations at present available will be dealt with in the communication which Mr. Bower will make at the meeting of the Linnean Society this (Thursday) evening. While every merit must be attributed to Mr. Druery for the first observations of this important fact, he has with great liberality allowed Mr. Bower free liberty to discuss the histological and theoretical points involved.

The obvious possibilities of discovery with regard to the reproduction of ferns may now be regarded as exhausted. It may be interesting to give the dates of the different steps:—

1597	Gerarde	...	Observed seedling plants near parents.
1648	Cæsius	...	Sporangia.
1669	Cole	...	Spores.
1686	Ray	...	Hygrosopic movements of sporangia.
1715	Morison	...	Raised seedlings from spores.
1788	Ehrhart	...	Prothallium.
1789	Lindsay	...	Germination of spores.
1827	Kaulfuss	...	Development of prothallium.
1844	Nägeli	...	Antheridia.
1846	Suminski	...	Archegonia.
1874	Farlow	...	Apogamy.
1884	Druery	...	Apospory.

Royal Gardens, Kew W. T. THISELTON DYER

MODERN ENGLISH MATHEMATICS¹

YOU will remember that two years ago it was announced from this chair that the Council had settled the conditions under which the De Morgan Medal should be given, and that the first award would be made at the anniversary meeting of 1884.

I have now to make the announcement that the Council has decided that the first medal should be given to Prof. Cayley, in acknowledgment of his work in the theory of invariants.

As this is the first award of the medal, I may remind you of its origin. Soon after the death of De Morgan, some of his admirers started a subscription for the double purpose of having a bust executed and founding a medal to be given in his memory. The bust now adorns the library of the London University, where also his valuable collection of books is preserved. The medal was offered to the Mathematical Society, and its Council accepted the honourable duty of determining its award. There is a peculiar fitness in the medal being thus connected with our Society; for this Society was founded with the active co-operation of De Morgan by a number of his advanced students, among whom his talented son George, who died soon afterwards, took the lead. De Morgan himself was the first President, and our *Proceedings* begin with a very characteristic opening speech by him.

The medal is to be given for eminent original work in mathematics, and no more fitting memorial than this could in my opinion be devised for a man who spent his whole life in carefully preparing the foundation for such work by his teaching and his writings.

De Morgan was pre-eminently a teacher. His most original work does not so much increase our stock of mathematical knowledge, but is concerned with mathematical reasoning, and with exact reasoning in general.

In the opening speech referred to, De Morgan himself divides exact science into two branches, *the analysis of the necessary laws of thought*, and *the analysis of the necessary matter of thought*. His own work belongs to the former. He was a logician much more than a mathematician in the ordinary sense of the word, and when reading his mathematical works I have always had the feeling that he studied mathematics not so much for his own sake as on account of the logic contained and exemplified in it. I once made this remark in the Professors' Common Room of University College, when an old colleague of his turned round and said, "You are quite right, he told me so himself."

In this work De Morgan did not stand alone. We may almost take him as a type of his period. It has often struck me as a noteworthy fact that in England, after the long pause in mathematical activity, the work taken first in hand was investigation into the very bases

¹ An address delivered by Prof. Henrici, F.R.S., at the annual meeting of the London Mathematical Society (November 13), on the occasion of presenting the De Morgan Memorial Medal to Prof. Cayley, F.R.S.